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In the same way it can be shown that the effect of the 427-day period for the years 1890–1898 is as follows :—

	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.	1898.
	$''$	$''$	$''$	$''$	$''$	$''$	$''$	$''$	$''$
x =	$-·09$	$-·15$	$-·11$	$+·01$	$+·11$	$+·14$	$+·07$	$-·04$	$-·13$
y =	$+·12$	$+·03$	$-·10$	$-·13$	$-·08$	$+·03$	$+·11$	$+·12$	$+·05$
z =	$+·02$	$-·01$	$-·01$	$-·02$	$-·02$	$·00$	$+·02$	$+·02$	$+·01$
	s	s	s	s	s	s	s	s	s
$\frac{x}{15 \sin \epsilon}$ =	$-·016$	$-·026$	$-·019$	$+·001$	$+·018$	$+·023$	$+·011$	$-·007$	$-·022$

The effect of the 427-day period is therefore to produce a seven-year period in the observed corrections to the right ascensions of the stars derived from the observations of the Sun, as also in the deduced value of the obliquity (y) similar in amplitude, but varying in epoch, whereas z or the observed distance from the pole to the ecliptic, is not sensibly disturbed.

The 427-day period, as determined by Dr. Chandler, is liable to small fluctuations in amplitude and period, and therefore the above corrections are liable to similar changes for other dates.

On the "Two-Method" Personal Equation.
By Walter W. Bryant.

For several years past it has been part of the routine at Greenwich for the transit observer for the night to observe occasional clock-stars by the eye-and-ear method, so that, in the event of a breakdown of the chronograph or of the observing circuit, all the observers might be in sufficient practice to be able to take good observations by the older method, and also that their personal equations, when using that method, might be approximately known.

Before 1892 it was the rule to deduce a separate clock-error from stars observed eye-and-ear (generally not more than two by one observer on the same night), and to compare this with the clock-error deduced from all the galvanic observations of clock-stars on the same night by the same observer. But as this involves an extra element of uncertainty in the comparison, it was decided in 1892 to compare only the observations made of the same star by the two methods on the same night, thus at once very much simplifying the comparison, and eliminating instrumental error.

Professor Safford has already made use of some of the results in papers presented to the Society, but, as my investigation starts from a different point, and as I am enabled, by the permission of the Astronomer Royal, to include some hitherto unpublished

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results for more recent years, I hope to add some interesting matter on this subject.

I had intended to deal with the whole period of six years during which the same rule has been followed, but I find it better to omit 1892, as, of the three observers who have worked regularly since that time with the transit circle, H. was absent on longitude work for a great part of 1892, and A. C. and B. were both in that year too recent as observers to have formed a steady "habit."

First let me note briefly what appears to me to be the psychological meaning of the interval under discussion.

In galvanic observations there are two known methods, called by Professor Newcomb methods A and B, and referred to by psychologists as muscular and sensorial. In the first the observer's attention is concentrated on effecting a coincidence in time between his perception of the bisection of a star, and his making the galvanic contact. In the second the observer waits until the moment of estimated bisection before making contact. Hence, omitting accidental errors, depending partly upon the galvanic current itself and partly on the clock, I should expect the time of a transit to be $t+t_s+t_c$ by method B, and $t+t_s$ by method A, t being the true time, t_s time occupied in seeing or "reaction to light," t_c in making contact; which in method A is to be theoretically zero.

In general, then, I should expect observers by method A to register a little late, and those by method B later still, by the addition of t_c.

In eye-and-ear observations a new suffix is introduced, t_h instead of t_c, where t_h is what the psychologists call the "reaction to sound," hence in this method I should expect, omitting as before accidental errors,

$$\text{Time of Transit}=t+t_s-t_h.$$

There are two methods of eye-and-ear observing, but I see no reason to suspect any difference in the results comparable with the known difference between the two galvanic methods.

Thus the two-method personal equation should be fairly represented for method A by $+t_h$ and for method B by $+t_h+t_c$.

It is to be expected, therefore, that, except in very abnormal cases, observers will make their eye-and-ear transits earlier than galvanic ones, and that those who adopt method B (of whom H. is a well-known example at Greenwich) will have a much larger discordance in the same direction.

It must be borne in mind in comparing the tables which follow that in general the transit-clock is not the same as the sidereal standard used for galvanic observations. Clock Hardy in the transit-room is practically the only one in use for the purpose of eye-and-ear comparisons, and Clock Hardy is a long-suffering machine, exposed to many climatic changes, and with people

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often passing to and fro, close to its face, so that it is not fair to expect a constant rate to be maintained. Hence it is vital that a comparison between the two clocks should be made as nearly as possible at the same time as the eye-and-ear observation, if the investigation is to deal with any confidence in very small intervals of time.

It is also important that such comparisons should be made always alike. Clock Hardy is provided with an automatic registration circuit for this purpose, but it sometimes fails owing to bad contacts, faulty insulation, or other causes. So that in many cases some hours elapse between the eye-and-ear observation and the Hardy signal, and in many other cases the signal is sent by hand, in which case the greater part of t_h is liable to disappear, thus reducing the apparent two-method personal equation.

This happened for a long period in 1893, owing to some defect in the wires, and will be referred to later.

It has also sometimes happened that the seconds of Clock Hardy have been alternately long and short, which would cause a systematic error to creep into observations of stars whose eye-and-ear interval is an even number of seconds or thereabout, while those whose eye-and-ear interval is an odd number of seconds would be practically unaffected.

Add to these many other probably small sources of uncertainty, and it will be seen that even if the number of comparisons is large enough to exclude unsystematic errors, it is quite possible that the results will be disappointing.

The following table gives the two-method equation for H., A. C., and B., in each of the first years under discussion, with the number of comparisons and the probable error of a single determination (which has been taken as the unit instead of a single night's determination) :—

	1893.	1894.	1895.	1896.	1897.
H....	$+^{s}.441$	$+^{s}.481$	$+^{s}.438$	$+^{s}.527$	$+^{s}.508$
No. of Stars ...	78	40	80	39	45
Probable Error ...	$\pm^{s}.062$	$\pm^{s}.082$	$\pm^{s}.086$	$\pm^{s}.085$	$\pm^{s}.053$
A. C.	$+^{s}.107$	$+^{s}.093$	$+^{s}.062$	$+^{s}.071$	$+^{s}.082$
No. of Stars ...	69	24	22	17	25
Probable Error ...	$\pm^{s}.065$	$\pm^{s}.051$	$\pm^{s}.059$	$\pm^{s}.065$	$\pm^{s}.072$
B.	$-^{s}.004$	$+^{s}.056$	$+^{s}.040$	$+^{s}.016$	$+^{s}.025$
No. of Stars ...	196	109	124	141	85
Probable Error ...	$\pm^{s}.064$	$\pm^{s}.057$	$\pm^{s}.051$	$\pm^{s}.046$	$\pm^{s}.041$

Mean values ... H. = $+^{s}.479$ A.C. = $+^{s}.083$ B. = $+^{s}.027$.

The relative smallness of the values for H. and B. in 1893, and for H. and A. C. in 1895, I am inclined to attribute to the

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failure of automatic registration mentioned above. With these exceptions there is no large annual variation, and, in fact, nothing that might not be accidental owing to some of the causes explained. I have, however, gone further into the subject from three points of view.

(1) Dividing each year into months.
(2) Dividing each day into 3-hour groups.
(3) Grouping the stars for every 10° of N.P.D.

The first of these is done for B. and H., the others for B. only, as there seemed to be insufficient data for a complete discussion of all three.

	H.			B.		
	Two-Method P.E.	No. of Stars.	Probable Error.	P.E.	No. of Stars.	Probable Error.
	s		s	s		s
January	+·443	12	±·099	+·034	53	±·055
February	+·425	24	±·090	+·004	44	±·066
March	+·454	30	±·063	+·015	68	±·060
April	+·451	31	±·075	−·021	53	±·053
May	+·456	39	±·081	+·022	69	±·055
June	+·471	26	±·071	+·031	42	±·058
July	+·520	19	±·113	+·040	31	±·051
August	+·455	22	±·062	+·022	66	±·050
September	+·498	17	±·055	+·032	57	±·046
October	+·489	19	±·056	+·021	65	±·055
November	+·500	21	±·076	+·043	77	±·056
December	+·502	22	±·089	+·023	38	±·042

G.M.T.	0^h	3^h	6^h	9^h	12^h	15^h	18^h	21^h
P.E. of B. ...	−s·003	+s·028	+s·006	+s·024	+s·030	+s·037	+s·034	−s·018
No.	3	45	160	166	125	89	48	21
Probable Error	±s·077	±s·062	±s·053	±s·055	±s·059	±s·040	±s·058	±s·055

N.P.D.	51°–61°	61°–71°	71°–81°	81°–91°	91°–101°	101°–111°	111°–121°
P.E. of B. ...	−s·002	+s·033	+s·020	+s·029	+s·019	+s·019	+s·006
No.	56	126	154	155	94	54	17
Probable Error	±s·064	±s·054	±s·050	±s·050	±s·051	±s·061	±s·070

As I anticipated in the early part of the paper the result should be persistently positive, it may be well to note the cases where it is negative, or so small as to be of doubtful sign. I find that a very large proportion of the observations included in B. February, B. April, and B. 6^h were taken in 1893. It has occurred to me to reject 1893, but the evidence is insufficient as to whether on any occasion the registration of signals was or was not automatic. So I have allowed it to stand.

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The cases B. 0^h, B. 21^h, and B. 111°–121° rest upon very few observations, and probably do not give a real mean value at all.

There remains only B. 51°–61°, but I find that a very unfair proportion of these are of *α Lyræ*, which brings me to another line of investigation.

It is said that there is an almost invariable tendency to observe (galvanically) bright stars relatively earlier than faint ones, possibly owing to a larger bisection error which may be systematic. Into this question, however, I have no wish at present to intrude, as it does not come within the scope of the paper. I only allude to it because if true the anomalous nature of the result, B. 51°–61°, might be fairly ascribed to the supposition that for *α Lyræ* in particular the galvanic time was early and hence the relative P.E. too small.

I should have been glad to carry out the comparison between groups of stars of different magnitudes; but there is practically no reliable evidence as to the observing conditions in each case, and as to the relative brightness of the same star under different conditions of daylight, twilight, darkness, and cloud.

One other field remains, not foreign to the subject. In observing slow-moving stars, I have practically no data to go upon, but, so far as my experience goes, stars right up to the pole should not give more widely different results between the two methods than the clock-stars for which the data are at hand.

For stars within a few degrees of the pole, I am inclined to think that the Greenwich method of galvanic observations with the very slight modification necessary to make it applicable to eye-and-ear observations, by substituting a known second from the clock for the galvanic contact simultaneously with which the bisection is made by means of the Right Ascension micrometer, would give practically the same result by either method.

A Note on the Result concerning Diffraction Phenomena recently criticised by Mr. Newall. By F. L. O. Wadsworth.

(*Communicated by the Secretaries.*)

In the November number of the *Monthly Notices* Mr. Newall has a note calling attention to an error in a result recently used by me in developing the theory of the "contrasting" or "delineating power" of telescopes. A criticism of this same result had already been published by Professor Schaeberle in the *Astronomical Journal* (No. 421). The criticisms of both Mr. Newall and Professor Schaeberle are just so far as the inaccuracy of the result alone is concerned; but they are both at fault as to the real error that was committed in obtaining it, and as to its influence on the conclusions of my former papers.

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